SCIENCE

FRIDAY, JULY 7, 1899.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson N. Y.

*KNOWLEDGE AND PRACTICE.**

THE honor of delivering the address upon this occasion is great; the responsibility of appearing as the successor of the distinguished men who have addressed you in previous years is also great, yet, as I thank you for your generous welcome, I feel, most of all, the pleasure of being the guest of Yale. To a Harvard man an honor bestowed by Yale has a special and very pleasant value.

Yale and Harvard have been working together for two centuries; their aims have always been similar; their developments have been parallel, and they have long sought one another for those friendly contests, intellectual and athletic, which yearly renew the close bonds between the two universities. I hope that their experience has been mutually helpful, for I am sure at least that Harvard has often learned from Yale, and they both have the same problems to solve if necessary.

Just at present there is a whole series of urgent problems in medical education before both institutions, and I shall, with your permission, try now to contribute to the discussion of some of those problems. You, who are upon the eve of graduation, know that you have received a far better preparation for the practice of medicine than was possible for any one to obtain a generation ago. You owe this advantage to the constant recognition of the possibility of improvement in medical education, and you should carry forth the feeling that it is now your duty to promote further progress in the organization and methods of medical schools. It is, therefore, eminently fitting

* Yale University Medical Commencement Address, June 29, 1899.

to the occasion to examine some of the possible ways of advancing medicine. Permit me, then, to lay before you certain suggestions which my experience as a teacher of medical science has brought to my mind. Others can deal far better than I with the strictly clinical problems. In the course of my address I shall have to emphasize certain limitations which are detrimental to medicine and which progress must step over. We must, however, not forget that criticism in itself is of slight value, unless it guides us to possibilities of progress. This interpretation of criticism should preside throughout our discussion.

The physician's work is not a trade in which he can perfect his skill once for all, but a profession based on learning, without interruption, new facts, new methods and even totally new ideas. The conversion of a student into a *medicinæ doctor* is too commonly looked upon as the end of the period of learning, but the student ought rather to look upon it as a certificate that he is at last qualified to learn, with reasonable efficiency, and, above all, with reasonable security as to his learning aright. Routine in medical practice is professional degradation.

There is one problem which we must all meet, the solution of which we cannot shirk, except by the supreme and final cowardice of suicide. So long as we live we are giving our solution to this problem, the problem of conduct, solving it equally by what we do and what we do not do, by our activities and our inhibitions. Some men give a mean solution, a mere summing-up of whims and accidents ; others give the bad solution of selfishness, passion or vice. The great object of our universities is to aid men to reach a noble solution under the dominion of wisdom and uprightness. You will often hear the assertion that our colleges have one of their most important functions in the building-up of character. The college of which this is not true deserves no students. But with this assertion is sometimes coupled the implication that the professional and scientific schools do not exert as much influence as the college on character. Such an implication may be excused to ignorance, but that would be a sorry medical school of which it were true. A medical school must develop character as well as mind, or else fail to produce graduates who can solve their problems of professional as well as of personal conduct.

Conduct presents to us a fourfold aspect : a physical, a social, an æsthetic and an intellectual. The physical aspect is that which the physicans chiefly deal with, it being their work to regulate understandingly the doings of the body. To fit men for this work is the object of medical education. How to acheive this object I will ask you to discuss with me presently. The social aspect of conduct, the relation of what the individual does in its bearing upon others, has endless phases, but there is no profession in which the personal social relations are so much a part of the necessary professional equipment as in the profession of medicine. The practitioner must abound in conscientiousness, honor and tact, and it is a natural consequence that there exists a code of ethics wherever physicians have formed associations. The æsthetic side of conduct, the securing of beautiful things because they are beautiful, concerns the medical man less, although he pursues the art of healing, and healing is really an art, a very fine art, as well as a science. The intellectual aspect of his own conduct is to the physician sovereign over all the rest. Medicine is distinctively an intellectual occupation. Let this bare schedule suffice to emphasize the fact that the physician more than men in most occupations needs a varied endowment, a broad foundation of character and a liberal education. I think that physicians as a class are distinguished

by the breadth of their sympathies and by their manifold interests. This is the natural consequence of their having to deal constantly, for themselves and for others, with the problem of conduct in all its aspects.

In contrast with all these liberal qualities we find too commonly an attitude toward pure science and toward biology which seems to me much the reverse of liberal. I must go farther and say frankly that this prevailing attitude marks an important limitation of the medical profession, and that it points to defects in our system of medical education. Courageous acknowledgment of our needs is the first step towards satisfying them. I shall now endeavor to convince you that the criticism made is not only just, but that it discovers the way for vast educational progress, and I trust that in the end you will feel the point of view to be both friendly and helpful.

The mental attitude, the modification of which seems to me so desirable, reveals itself by distrust of pure science and by ignoring the relations of medicine to biology. Let us consider the case in its two phases.

First, then, as to science. To the academic proposition that science is the basis of medicine every one would assent, but, none the less, a great many practitioners still draw a sharp line between 'theoretical' and 'practical' doctors. Those who think this distinction are, of course, 'practical' men, and they are guilty of a triple error: first, that scientific is theoretical; second, that theoretical is impractical; third, that practical is superior to theoretical. This misconception still exerts influence, although it is certainly waning. Make it your part to hasten its extermination.

The feeling against medical science as impractical has been very strong in America, probably stronger than anywhere else in the world. Our colonial ancestors had to help themselves in every thing, and we have apparently inherited the belief that any way is the best way, and that expert capacity is a *luxe de trop*. We must remember, too, that medicine grew up as an art, not as a science, and that such progress as it made until this century was well along was chiefly by empirical experience.

It is only for about fifty years that research, properly so-called, has been an important factor, only about twenty-five years that it has been the leading factor in medical progress. It is, therefore, not to be wondered at that older practitioners, especially if without intercourse with university centers, are unconscious of the full measure of the change. The change is momentous, yet it has been so rapid that it falls largely within the period of the medical experience of many here present. Another curious factor in establishing and maintaining the notion that science is unpractical was the conventional idea of a scientific man, which prevailed even within thirty years, and which I can perfectly recall as a half-accepted standard when I decided to choose science for my career. This conventional scientist was a man past middle life, who wore an unfashionable hat, large spectacles, ill-fitting clothes, who was more or less absorbed in abstruse ideas and in studies of no practical use, really learned, very absent-minded and more rather than less in need of being looked after by somebody with common sense. It was almost necessary for a scientific man to cultivate absence of mind in order to sustain his reputation. It would certainly be an interesting study to trace the history of this phase of American science, since it was a phase, though a quaint one, of the eternal assertion that the best science is independent of immediate utilitarian consideration.

Resuming the direct course of our thought, we may say that on the one side the notion that 'scientific' is synonymous with 'unpractical' has its historical justification, but

on the other side it is now only a survival, an unjust opinion, a prejudice to be surrendered unconditionally. The prejudice against science has been very influential. Even fifteen years ago a young physician could not afford, no matter how much leisure he might have, to work in a scientific laboratory, because he would have been stigmatized as 'theoretical' and patients would have been turned away. I speak by chapter and verse, for I know many instances of young men beginning good research work, and then soon being compelled to give it up, by the force of professional opinion. We now know that this opinion was in large part a prejudice, the disappearance of which removes the final barrier across the entrance to the new era, which it has taken our entire century to open. The establishment of science in its rightful place has been going on steadily for a long period, but within five years it has rushed towards its culmination. We owe the complete medical recognition of the value of pure science to Bacteriology. Listen to the following dates. In 1879 Koch introduced the method of solid cultures; in 1882 he published his monograph announcing the discovery of the bacillus of tuberculosis. In 1884 came Löffler's paper on the bacillus of diphtheria. In 1891 appeared Councilman's account of the amœba of dysentery. At the International Medical Congress in 1893 Roux described the use of antitoxine in diphtheria, and about the same time McFadyean secured recognition for the value of mallein in the diagnosis of glanders. In 1896 came Vidal's reaction for identifying the germs of typhoid fever.

Here were results entirely beyond the ken of the practitioner, laboratory discoveries which he could only accept but not verify for himself, although in their application he could furnish dramatic proof of their value. No wonder that science now receives her meed.

It is safe to prophesy that hereafter medical science and medical practice will be both more sharply divided and more intimately correlated than heretofore. We already note that the experience of the clinician can rarely do more than effect improvements in methods, while the new principles come from the laboratories. The clinician may ask good questions, but he now depends so much upon the scientific worker for his answers that there is a sudden demand for laboratories in connection with every hospital. This demand marks the final unconditional surrender of the practitioner to pure science. The end is not yet. It is not enough that the value of scientific research is at last acknowledged, but the practitioner must also adopt the scientific method for himself.

What do I mean by the scientific method? There is much vague misconception concerning it, based upon the erroneous assumption that it is a peculiar method belonging to science. It is really only the right method of ascertaining the objective truth. It is in the classic words of von Baer, *Beobachtung und Reflexion*, observation and reasoning. The student at the microscope looking at nuclei and protoplasm and deriving therefrom a correct conception of the nature of a cell uses the scientific method, and he uses the scientific method again when he observes the symptoms of patients and reasons therefrom. There is nothing to distinguish the scientific method from the methods of every-day life except its precision. It is not a difference in kind or quality, but a quantitative difference, which marks the work of the true scientist and gives it validity. The definition of the scientific method seems simplicity itself, nevertheless it takes years upon years of the severest discipline to give even a partial mastery of the method, because to observe correctly and reason correctly are the most difficult accomplishments a man can

strive for, and he who acquires them to a high degree is a great man; such were Helmholtz, Darwin, Newton and a few others out of all time. The two grades of observation and reasoning must be distinguished, the lower repetitive grade and the higher original grade. Many a person of ability may be taught to see and understand that which has been seen and understood before. Such persons in medicine can make correct diagnoses of known diseases, but in the presence of the new unknown they fail. Such persons in science may do good work as followers, not leaders—privates, not generals. To the few are accorded the privileges of the higher grade, right sight and right thought as they invade the unknown. The training in exact science does more than any other discipline to elevate those who have sufficiently great gifts into this highest intellectual grade. We say, therefore, unhesitatingly that severe scientific education is the principal addition we ought to make to our medical curriculum. So I come back to my opening assertion: We must teach how to learn, and how to learn from the unknown.

If we admit the principle that science should have a more influential place than at present we must decide in what way that place can be provided. It is, I think, undesirable to lengthen the medical course beyond the four years now required; it is undesirable to omit any of the subjects now offered, and it is equally undesirable to enlarge greatly the fundamental scientific courses in anatomy, physiology, pathology, etc. We seem surrounded by impassable walls, but there are two considerations which may guide us. On the one hand is the enormous growth of medical knowledge, which is beyond the power of any single student to master, so that some choice must be made for him or by him. On the other hand the science we are now seeking a place for is not that which is basal, but that which is to perfect and end the whole training; it is to be the top, not the foundation. Clearly, then, the way out is to introduce the elective system on a large scale into the fourth and perhaps third year. Make a series of these electives for advanced work in scientific subjects, such as anatomy, embryology, physiology, pathology, pharmacology, bacteriology. As you know, this solution has been tried, and with most encouraging results. May we not look forward to its becoming the universal method throughout America?

As regards the elective system I follow Dr. Henry P. Bowditch in believing that it should be greatly extended, and that the required studies in medicine should be reduced to the minimum, and numerous electives provided for every year of study. These proposed electives may be in subjects already taught and also may provide courses not usually offered, such, for example, as examination of the blood, pathological chemistry and psychology in its medical aspects. The elective system is the educational answer to the tendency toward specialization in practice, and I believe that we have no choice as to its adoption.

We pass on to the consideration of the second phase of the case which we are debating. It will probably need a much longer and more sustained effort to bring about a correct recognition of the relations of medicine to biology than is needed to win recognition for science at large.

Medicine is one department of applied biology, just as dyeing is one department of applied chemistry, or electric lighting a department of applied physics. Now if a man wishes to become an expert dyer or electrician he studies chemistry as a whole and physics as a whole, but the would-be physician begins at once with human anatomy and human physiology, and probably to the end of his days never discovers that he has no conception whatever of biological

science. Carl Semper used to say, *die Mediziner sind lauter verdorbene Zoologen*—the medical men are all spoilt zoologists—and the saying still remains only too nearly true.

The first question is : What place shall be given to biology in medical education ? In order to answer this question we must remember that biology should here serve a twofold purpose, that of making the beginning so as to lay the proper foundation for further study and that of inculcating the value of the comparative method.

The fundamental principles of biology ought to be taught to every student of medicine before he is allowed to study medical anatomy or physiology. This great reform will surely come about, and has, in fact, been already effected by one important university, which has made biology a requirement for admission to its medical school. Or perhaps the necessary time can be secured, after the student has entered the medical school, by lessening the number of hours now required for anatomy. That far more time is usually devoted to anatomy than is advantageous to the student I am thoroughly convinced. Formerly, when gross anatomy gave the student almost his only training in exact scientific observation, the subject had a pedagogic value, which it has since lost in very large measure, because histology, experimental physiology, bacteriology and pathology offer far better discipline of the observational power than anatomy alone can supply.

It must be further remembered that a large part of anatomy is to the student sheer memorizing and without intellectual value. Finally, we all know that a large proportion of the facts of descriptive anatomy are speedily forgotten after the examinations are past, and that the practitioner finds no occasion to recall them. A study which occupies so many hours as to exclude other valuable forms of mental training and imparts much information not of practical value may well be abbreviated. On the other hand, a thorough course in descriptive anatomy, exclusive of histology and surgical anatomy, must always be indispensable. The only question is concerning the proportionate division of time with the other studies, which within recent years have become equally indispensable.

My second point is the inculcation of the value of the comparative method, to which the development of biological science is mainly due. Life presents itself in an immense variety of species, and the vital phenomena assume a characteristic manifestation in each species. It is by comparing the structure and functions that we are able to distinguish the fundamental and essential part of the phenomena from that which is secondary, and thus we gradually reach those generalizations which alone constitute true science. A detail is a grain of earth, useless for building until it is compacted with many other grains into a useful shape, which, hardened, like a brick, in the furnace of thought, can be added to the temple of knowledge. Now, since medical interest centers in man, medical investigators have cared little for comparative research, and have often failed to grasp the problems with which they dealt. Many an able physician, when he studies, say the physiology or pathology of a dog, a guniea pig or a frog, honestly thinks that he is studying comparative physiology or comparative pathology, although he is really doing nothing of the kind. He is studying, perhaps, gastric digestion or the hypertrophic degeneration of the liver; he seeks to understand the process in the one organ or the other, and the stomach is to him the stomach, the liver the liver. He may note the differences between one animal and another if they are marked, but he does not attempt to determine the process in the carnivora, the rodents and the amphibia, see what is common to them all, and what is special modi-

fication for each of these groups. One has only to read any accepted text-book of physiology or pathology to see that it is absolutely true that the narrow or anthropomorphic view is the typical medical view. The medical man may learn from the zoologist and botanist, who have depended chiefly upon the comparative method for their most important results. Science cannot be hampered by any conventional restriction; it must be free to turn in every direction in which a discovery is possible. Now, medicine places a conventional restriction around the medical sciences, for by custom and precedent it orders that, even though the actual investigation be upon some animal, it shall be regarded solely as elucidating human structure and human function—in other words, the interpretation must be anthropomorphic. This convention has led to some strange absurdities, of which I shall mention only one; the microscopic structure of the kidney has been investigated chiefly in animals, notably the dog and rabbit; all the text-books of anatomy and histology known to me, with a solitary exception, describe the structure of the human kidney in accordance with the observations on these animals; but, as the human kidney really differs in many important respects from that of the dog and the rabbit, the structure of the *human kidney* still remains generally unknown. This error has been perpetuated through fifty years. Since zoologists are habituated to the comparative method, would it not be wholly impossible for a blunder of this kind to be kept up in their work?

I am so thoroughly convinced of the value of the comparative method, of the absolute necessity of its adoption in medical research, that I look forward to its acceptance as the greatest advance in medicine which our time will know. Methods of obtaining knowledge are the means of progress. Remember how much anatomy owes to the method of human dissection; how much pathology owes to the method of staining microscopical preparations; how much surgery owes to the method of antisepsis; how much bacteriology owes to the method of artificial cultures. These are, however, merely technical methods, but that which I am now advocating is a mental method, a way of successful thinking, a process of right reasoning, far more comprehensive than any technical method; and, if we accept it, we can explore vast regions of knowledge, the very possibilities of which we barely recognize now. Let it encourage us that the comparative anatomist and comparative embryologist are already well advanced along the path which the physiologist and pathologist must now learn to follow.

Medicine is destined to become comparative, because it must advance. The wise action for us is to facilitate that advance, and thus the question becomes: What shall we do practically to establish and promote comparative medicine? If we agree that our aim is to secure the very best kind of research in medical science the practical answer is clear: We must provide post-graduate instruction, with courses thoroughly systematized and correlated, covering at least two years, to qualify men to become professional investigators in the comparative sciences of morphology, physiology, pathology, bacteriology, preventive medicine, etc. It is remarkable that these sciences have never reached a university standing. It ought now to be secured. If a young man wishes to make a scientific career, if his interest is chemistry, physics, botany or zoology, he is received at one of our universities started upon a well-planned course properly systematized, he gives for two or three years most of his strength to his main subject, but he follows probably two cognate subjects as minor studies, and at the end of his time, if successful in his

work, he receives a degree, which attests his proficiency in his special science. Should the same young man elect to study one of the medical sciences, physiology, pathology or bacteriology, no university will give him corresponding recognition. The utmost he can find is opportunity for advanced work in his special subject, but with no university guidance, no plan of correlated studies, and he can look forward to no degree nor even to a certificate from the university. Must we not admit that here is a great omission in our university organization? Is it not a pressing duty to repair this omission? Surely to put these questions is to assent to them.

We are thus brought to the conclusion that, though the primary function of our medical schools is to educate practitioners of medicine, yet they ought to assume now the further and higher function of training medical investigators. To succeed in this the medical laboratories must be expanded, their resources enlarged and the staff increased, so that the officers will have time and means for both researches of their own and for guiding the researches of advanced students. Yale has been teaching a needed lesson, for her laboratory of physiological chemistry has shown what splendid results ensue when one of the so-called medical sciences is set free and allowed to develop as the peer of other sciences. Untrammel them, strike off their bonds, and comparative morphology, comparative physiology and comparative pathology will develop and add to the good work and glory of your alma mater as physiological chemistry has already done.

Laboratories are of very recent origin; seventy-five years ago there were none. There are but few laboratories which have stood for as much as twenty-five years. Our experience with them has not been long, but we have learned two things concerning them: that they are absolutely indispensable and that they are very costly, so costly that a university has become an enterprise of great financial magnitude. Formerly a college with an endowment of a million dollars was wealthy; at present a university with three thousand students and twenty millions dollars has to practice rigid economy to keep running properly. We who are at work for universities are painfully conscious of needs, and it seems to me a common duty for us all to make known to the public, upon whose generosity American higher education depends, the true scale of those needs.

The requirements of comparative medicine call for more changes than we have yet mentioned. The very word comparative implies that animals shall be included in the range of study. It means that not only shall provision be made for investigating the structure of animals and for physiological experiments, but also for the observation and treatment of sick animals, or, in other words, there ought to be a veterinary hospital in intimate association with the school of human medicine. Such a hospital would increase the range of experience and contribute a broadening impulse to all medical work. It is, I believe, quite a new project to consolidate the interests of veterinary and human medicine, but it is, by the initiative of President Eliot, under actual consideration at Harvard, and will, if carried out, be an epoch-making advance. It will be a public and effectual assertion of the solidarity of all medical science and of all forms of medical practice. It will be a boon to pathological and clinical research, for it will offer opportunities for the study not only of diseases specially characteristic of animals (such as the distemper of dogs), but also of those diseases common to man and animals. We are thus brought round to still another aspect of the beneficence of medical consolidation, the future development of preventive medicine.

Preventive medicine is a term of recent currency. We have come to think more about it in consequence of the growth of our knowledge of disease-germs, which has led to the hope that we can control germs, so as to prevent or at least greatly diminish the danger of infections. Moreover, serum-therapy, the anti-toxin treatment and the discovery of the influence of the thyroid and suprarenal extracts have made us familiar with the conception that profound influences may be exerted by quantitatively slight changes in the chemical conditions within the body. Here are two illustrations of ways in which disease may be impeded. It is a field which might be considered a part of that of hygiene, but it is logically distinct. To stop disease is not the same as preserving health. Now, we are all agreed that prevention is a rapidly increasing part of medical practice, and, since many diseases, like tuberculosis, typhoid fever or the bubonic plague, are spread by animals, it follows that we must look upon the study of diseases of animals as an integral and indispensable portion of preventive medicine. A hospital is as necessary for the observation and treatment of sick animals as of sick men.

Most of us, I am sure, anticipate in the near future a magnificent development of preventive medicine. One of the best means to promote the fulfilment of these anticipations is to bring the veterinary hospital into close and intimate union with the medical school.

The veterinary profession, like the medical, is raising its standards rapidly, and we can only wish success to these efforts, for not only does the case of sick animals require the highest skill, but also the advance of veterinary science calls for the best scientific ability. If veterinary schools are brought, by means of their hospitals, into close touch with medical schools it will hasten the elevation of the veterinary profession, and will bring nearer the time we all shall say that the veterinary school is as worthy a place in the university organization as is the medical school. When that time comes, as the foundations of medicine will be broad and wisely laid, so will the superstructure be stable.

As for the fear, which I heard expressed at a recent medical meeting, that doctors are destroying their own means of livelihood, because preventive medicine is limiting the supply of diseases to be cured, I may say that fear has not limited the eagerness of physicians to increase prevention. On the other hand, there is the consoling hypothesis that there are likely to remain many diseases, especially those which are difficult to identify and to treat and also those of sporadic occurrence, which will keep practitioners busy in the future. As the time is passing away when a large part of active practice consisted in cases of typhoid, diphtheria and other preventable diseases, rarer forms of illness will be more thoroughly studied, and, as they will require a higher skill, the future physician will seek a better training than we can offer to-day. Thus one of the indirect results of the advance of preventive medicine is to raise the standard of medical education.

I have said enough to indicate the far-reaching consequences of the conviction, which I hold and hope you hold, that the comparative method of biology is to direct the development of medical practice. The adoption of the comparative method will revolutionize both medical teaching and the organization of our medical schools.

We must now turn our attention to certain other questions of medical education. During the past year there has been going on a very widespread discussion in this country over the curriculum for medical students, and the prospect of consequent improvement is encouraging. I cannot venture upon attempting more than the

presentation of certain definite ideas which have formed themselves in my own mind as the result of the late discussion, and must leave to others a more comprehensive treatment.

Foremost in importance is the idea that the number of lectures is too great, probably, in every course given, and that the laboratory work and the personal clinical work occupy too small a proportion of the student's time. The practical work is the instructive work; it is the source of real knowledge. The actual direct contact with the objects and with the phenomena *is* knowledge. The very best that can be said of a lecture or a book is that it describes well the knowledge which someone possesses. There is no knowledge in books, and that motto ought to be inscribed over the library door. A book or lecture can serve only to assist a man to acquire knowledge with lessened loss of time. Knowledge lives in the laboratory; when it is dead we bury it, decently, in a book. Now real knowledge is what the medical practitioner needs, the personal mental image of things seen, felt and heard; he needs to establish a short circuit between sensations and the true psychic concept, but if you train him to interpolate books you are likely to make the circuit so long that there will be no true concept at the end of such a resistance path. Our greatest discovery in scientific teaching is the discovery of the value of the laboratory and its immeasurable superiority to the book in itself. A lecture is a spoken book, and must, therefore, also yield to the superior claims of first-hand knowledge.

It is the corollary of the value of laboratory instruction that the examinations should be practical, or, in other words, that the conventional written examination should be given up. All the clinical work is, of course, to be classed as laboratory instruction, and the time ought not to be far distant when students will be required to make diagnoses from patients directly as the test of their proficiency. No one who has examined students in both ways is likely to question the superiority of the practical examination over the written. It is a real test of real knowledge, and is fair to the student for that very reason, and it avoids the two defects of the old-fashioned examination paper: first, the defect of testing memory rather than mental power; second, the defect of offering rewards for cramming. A practical examination has the great advantage of emphasizing to every student the necessity of personal familiarity with the objective basis of his studies.

A second important idea is that the requirements for a medical degree shall no longer be uniform for all candidates. That this idea will be adopted is necessarily the belief of every one who advocates the elective system.

A new arrangement of studies has been adopted by the Faculty of the Harvard Medical to go into effect next year. It is the result of prolonged careful debate. It is based upon three leading principles, concentration, correlation and sequence of subjects. The system consists in a division of studies by half years and by half days within the half year. The elementary anatomy will be confined to the first term of the first year, but will occupy half of every day; the other half of every day will be occupied by histology, embryology and a special course on the brain. In the second term a similar system will be followed, half a day for physiology and half a day for physiological chemistry. In the first term of the second year this simple dual plan is pursued with pathology and bacteriology; beyond this the arrangement is more elaborate, and for the third and fourth years is not yet fixed.

The new plan is, of course, an experiment, but is fully expected to prove a successful one, because it will make the work

of the student easier by concentrating his thoughts upon one subject instead of dissipating his attention among many subjects. If a man wishes to accomplish intellectual labor he seeks instinctively to apply himself wholly to that one task until it is completed. The capacity for sustained effort is the power by which the man surpasses the child. The child needs constant change and variety, and the system, which we have had in our school, of running from one lecture to another and from one laboratory to another, appears to many of us more suitable for school children than for young men studying medicine, and we expect, therefore, the new plan of studies to be justified by its results.

Here we must pause, although we have merely touched upon general principles and looked at a few details as illustrations. It seems to me that the whole problem of medical education is just now one of the most interesting and important ever presented in the history of American universities. If I have stimulated your interest in it I am rewarded.

Before I close I will venture to address to those of you who are to-morrow to receive your medical degrees a few words upon the deeper signification of your profession. This is not the time to enter into a discussion of the assumed antagonism between practical science and Christian faith. Each year brings the two into closer and more helpful relationship and increases their mutual understanding. The dignified agnosticism of Huxley and the lofty spiritualism of Brooks meet in the common conviction that the growth and development of man to a higher and better physical and spiritual life is alone what makes existence worthy.

We are living in an epoch of great scientific discovery and of consequent material progress, which among its many results includes numerous new facilities for intercourse between nations. In contemplating these facilities one recalls how great a part the free intercourse under the great Roman Empire played in the first spread of Christianity, so that one involuntarily asks: Is not science now aiding the same cause in a similar way? Science does more. By its steadfast pursuit of truth; by its broad-minded ability to acknowledge the truth whatever found; by its freedom from narrow dogmatism on the one hand, and from ignorant materialism on the other, science can do a noble work in the great battle between good and evil in the world.

The antagonism of science and religion is unreal. Our intellectual Quixotes take it for one of their windmills, but I very much doubt if it be more than the phantom of a windmill. When you, young men, begin your life's campaign, fight real foes, be blind to threatening phantoms and deaf to their noisy shibboleths. Attack real difficulties. Remember always that as physicians you will have to help others, and that it will be peculiarly your obligation to uphold the standard of faithful service and to defend what I may call the creed of science: that the advancement of knowledge is a duty because it serves mankind. Faithful scientific research is Christian service.

CHARLES SEDGWICK MINOT.

LORD KELVIN'S ADDRESS ON THE AGE OF THE EARTH AS AN ABODE FITTED FOR LIFE.

II.

A THIRD line of argument relative to the habitable era of the earth is drawn from the theoretical age of the sun. After stating the probability that, if sunlight was ready, the earth was ready both for vegetable and animal life within a century, or at least a few centuries, after the consolidation of the earth's surface, Lord Kelvin inquires whether the sun was ready, and re-

plies:* "The well-founded dynamical theory of the sun's heat carefully worked out and discussed by Helmholtz, Newcomb and myself, says No if the consolidation of the earth took place as long [ago] as 50 million years; the solid earth must in that case have waited 20 or 50 [30?] million years for the sun to be anything nearly as warm as he is at present. If the consolidation of the earth was finished 20 or 25 million years ago the sun was probably ready, though probably not then quite so warm as at present, yet warm enough to support some kind of vegetable and animal life on the earth." Here is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its *correctness*, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a *complete* theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. The Helmholtzian theory takes no cognizance of latent and occluded energies of an atomic or ultra-atomic nature. A ton of ice and a ton of water at a like distance from the center of the system are accounted equivalents, though they differ notably in the total sum of their energies. The familiar latent and chemical energies are, to be sure, negligible quantities compared with the enormous resources that reside in gravitation. But is it quite safe to assume that this is true of the unknown energies wrapped up in the internal constitution of the atoms? Are we quite sure we have yet probed the bottom of the sources of energy and are able to measure even roughly its sum-total?

*SCIENCE, May 19, 1899, p. 711.

There are some things hereabouts in the instruction we receive that puzzle us with our geological limitations:

1. We are taught that there is a certain critical temperature for every substance above which it takes the gaseous form, and no amount of pressure can reduce it to the liquid or solid state.

2. We are taught that gases are compressible to an indefinite extent provided their temperatures be above the critical point.

3. We are told the temperature of the interior of the sun is probably above the critical temperature of any known substance, and hence that all the material of the interior of the sun is probably gaseous.

4. We are taught that so long as the substances of the sun remain in the gaseous condition the temperature of the sun must rise from increased self-compression. It cannot, therefore, fall to the critical temperature of the component substances, and must, therefore, continue in the gaseous state and grow hotter and hotter.

5. We are taught that gravity varies inversely as the square of the distance. As the distance between any two particles is halved, their mutual attraction is raised fourfold. Perpetual halving would cause the attraction to mount up toward infinity.

In the sun, then, there seems to be this interesting combination : (1) a gaseous mass already above the critical temperature growing hotter and hotter by self-compression and bound to grow hotter and hotter so long as it remains a gas ; and it is bound to remain a gas until it falls below the critical temperature, which it cannot do while it continues to grow hotter ; (2) a gravity that increases four-fold with every halving of distance and that is bound to increase so long as concentration continues, and concentration must continue while the substance is a gas and the gravitative pressure increases.

What is the logical outcome of this kind of logic and this sort of a combination ? A geologist begins to grow dizzy contemplating such thermal possibilities. Why should not atoms, atomecules, and whatever else lies below, one after another have their energies squeezed out of them ; and the outer regions be heated and lighted for an unknowable period at their expense ?

There was a time when the chemical theory of the sun's heat was fairly satisfactory to the scientists of the day, but its inadequacy appeared in time. There followed a period in which the meteoroidal theory of the sun's origin was deemed adequate, but its defects soon became apparent. There has followed the contractional theory, the validity of which is perhaps not less questioned now than was the validity of the chemical and meteoroidal hypotheses in their day of acceptance, but, judging from the past, it may easily appear in the future that the Helmholtzian theory is inadequate in some measure not unlike its predecessors.

But assuming, as we are wont to do, that the limits of our present knowledge are a definition of the facts, has the evolution of the sun been worked out with such definiteness and precision as to give a determinate and specific history of its thermal stages from beginning to end? It is one thing to tell us, on the basis of the contractional theory, that the total amount of thermal energy originally potential in the system is only equal to so many million times the present annual output, but it is quite a different thing to give a specific statement of the *actual time occupied by the sun in the evolution and discharge of this amount of heat* and to define its successive stages. It is with this actual history that we are specially concerned. The distribution of the computed heat in time may have been such hypothetically as to shorten the period of its expenditure not simply to 20 or 25 millions of years, as indicated by Lord Kelvin, but to four or six millions of years as deduced by Ritter.* On the other hand, the dealing-out of this amount of heat may hypothetically have occupied a period many times the 20 or 25 million years postulated. It seems altogether necessary to determine specifically *the distribution of the heat in time* before any approach to a satisfactory application to geological history can be made. The period of 20 or 25 million years named can have little moral guiding force until this problem is solved. But the literature of the subject shows an almost complete neglect of this consideration. While certain of the physicists and astronomers have been instructing us '*e superiore loco*,' they seem, with very rare exceptions, to have overlooked this vital factor in the case. Even in computing the sum-total of heat they have, for the most part, heretofore neglected the central condensation of the sun and in their computations have sub-

* *Astrophysical Journal*, December, 1898 ; *Journal of Geology*, p. 93, No. 1, Vol. VII., 1899.

stituted a convenient homogeneity. This is recognized in a more recent number of SCIENCE (May 26) in the article by Dr. See, in which he offers a correction which involves an extension of the previously assigned output (18 million times the present annual radiation) to about 32 million times the annual radiation. But even in making this correction he neglects to consider the distribution of this heat in time, and leaves upon the reader the impression that the life-history of the earth was limited to 32 million years. Assuming the correctness of his computations, the past thermal discharge of the sun is merely limited to 32 million times the present annual expenditure. For aught that appears to the contrary, the actual output of this heat may have been spread over any assignable number of years. It is obvious upon consideration that a certain distribution of this past heat would favor longevity of life upon the earth, provided it could exist with a more limited heat supply than the sun is now yielding. On the other hand, it is equally evident that if the supply be distributed in certain other ways, either in the nature of excessive prolongation or of excessive concentration, the life era will be shortened. Doubtless the admonitory physicists have assumed that it was sufficient for the gross purposes of restraining geologists within due limits to determine the total amount of heat without assiduously considering the actual facts relative to its distribution, but some of us are unwilling to accept this loose method of dealing with the problem, since there are resources of application of which our physical friends have perhaps not taken cognizance. For example:

1. If at a certain stage in the evolution of the sun it occupied essentially all the space within the earth's orbit, and was giving forth one-half as much heat per year as now, it would possibly have sufficed for the needs of life upon the earth essentially as well as at present, without the assumption of any change in the constitution of the earth or of its atmosphere. For, on this supposition, approximately one-half of the space into which the earth radiated its heat would be blanketed by the sun and the heat thrown forth from the earth would be measurably caught and returned, and hence the loss of heat by radiation from the surface of the earth would have been reduced.

2. If, at the same time, we suppose that the material now concentrated in the outer planets was dispersed in a broad nebulous or meteoric belt mantling the heavens on the opposite side, another means would be provided by which some portion of the heat radiated away would be caught and returned to the earth, and a further small reduction in the original receipt of heat from the sun may be made consistently with the existence of life. This outer belt would be very tenuous and its effects correspondingly meagre, but it is a factor to be considered in a complete set of assumptions.

3. If, in addition to this, we make the consistent assumption that many other bodies of the heavens which are now concentrated into suns or into dark bodies were then in a more dispersed nebulous or meteoroidal condition, the general space of the stellar universe would be partially mantled, and there would be less free scope for the escape of the heat, solar and terrestrial alike, which is now freely lost through the open regions of space. It may be conceived that there was a common blanketing of the heavens by the dispersal of its now concentrated matter. This conception is the logical companion of the supposed dispersal of the solar matter. If the volume of matter in the stellar universe could be supposed to be sufficient, it might be so distributed hypothetically as to mantle the whole heavens and largely prevent the escape of central heat outwards, just as the

central heat of the more concentrated bodies is conserved at the present time. Under this conception the history of the stellar universe may be characterized as a progressive clearing-up of nebulosities and meteoroidal dispersions and the concentration of its matter about certain points, leaving between vast open spaces through which heat is now radiated away with a facility unrealized in the earlier stages. The quantitative value of such a suggestion must be left to the determination of astronomers who have the best data for forming a conjecture as to the ratio of matter to space in the stellar universe and as to the possibilities of its dispersion at a period coincident with the earlier stages of the earth's history.

4. A modification of the conditions assumed in the foregoing paragraphs may be postulated in which the earth is regarded as having made its early growth *within* the primordial meteoric aggregate, perhaps a great flattened meteoric spheroid, which initially extended beyond Neptune in nebular fashion and whose present attenuated representative may, perhaps, be found in the zodiacal light. In this case the thermal environment of the early earth was that furnished by the interior of the spheroid, though far out from the center. The conditions only became external gradually as the growth of the planets exhausted the peripheral portion of the meteoric spheroid.

5. The foregoing hypotheses, which do not seem to be so completely out of accord with the possibilities of the case as to be inadmissible tentatively in the absence of a positive solution of the early terrestrial environment, are concerned with the external relations of the earth. If we turn to the earth itself it may be remarked that the nature of its atmosphere very radically conditions the amount of heat requisite for the support of life. Dr. Arrhenius has recently made an elaborate computation relative to the thermal influence of certain factors of the atmosphere and has arrived at the conclusion that an increase of the atmospheric carbon dioxide to the amount of three or four times the present content would induce such a mild climate in the polar regions that magnolias might again flourish there as they did in Tertiary times. On the other hand, he concluded that a reduction of less than 50% would induce conditions analogous to those of the glacial period of Pleistocene times. The vast quantities of carbon dioxide represented in the carbonates and carbonaceous deposits of the earth's crust imply great possibilities of change in the constitution of the atmosphere of the earth in respect to this most critical element.

6. But there are more radical considerations that relate to the early thermal history of the earth. To be sure, if we are forced to adopt the hypothesis of a white-hot liquid earth, with all its extravagant expenditures of energy in the early youth of the earth, we can take no advantage of these possible resources, but under the supposition that the meteorites gathered in with measurable deliberation, it is theoretically possible to find conditions for a long maintenance of life on the earth, with little or no regard to the amount of heat which the early sun sent to it. In the earliest stages of the aggregation of the earth under this hypothesis, while it was yet small, it can scarcely be supposed to have been habitable, because its mass was not sufficient to control the requisite atmospheric gases, but when it had grown to the size of Mars, that is to a size representing about $\frac{1}{10}$ of its present aggregation, or, to be safe, when it had grown to twice the size of Mars, or about one-fifth of its present mass, it would have been able to control the atmospheric gases and water, and, so far as these essential items are concerned, it would have presented conditions fitted for the presence of life. At this stage the

larger portion, four-fifths by assumption, of the matter of the earth would yet be in the meteoroidal form and doubtless more or less closely associated with the growing nucleus. If the infalling of this four-fifths of the material of the earth were duly timed, so as to be neither too fast nor too slow, it would give by its impact upon the atmosphere of the earth a sufficiency both of heat and of light to maintain life upon the surface of the earth. The plunging-down of these meteorites upon the surface might be more or less destructive to the life, but only proportionately more so than the fall of meteorites to-day. It would not be necessarily fatal to life, especially oceanic life; indeed, the strokes of the meteorites might not be more inimical to the perpetuity of any given form of life then than are the attacks of its numerous enemies to-day. It was only another form of jeopardy. The latitude as to variation of rate of infall would be rather large. The infall must not have been so rapid as to have given a universal surface heat above 100° C. The life of hot springs crowds close upon this upper limit, as Lord Kelvin has indicated. The infall must not have been so slow as to have permitted the surface heat to fall universally below 0° C., making allowance for other sources. These other sources might have permitted the meteoric supply to fall considerably below the quantity represented by a surface temperature of 0° C. Between this indeterminable low point and a supply equivalent to 100° C., similarly qualified, there is a quite wide range. Those who have insisted upon the precipitate infalling of meteorites at such a rate as to reduce the earth to a nebulous condition will probably not feel entitled to doubt the adequacy of this source of light and heat. They can only question the possibility of the meteorites falling in slowly enough to permit the coincident presence of life on the earth.

This hypothesis starts life at a period when the earth was one-fifth grown and prolongs it throughout the rather slow gathering-in of the last four-fifths of the earth's mass, and hence gives to the earth a long era of autogenic life conditions.

Now, if a hypothesis relative to the early constitution and the growth of the rest of the solar system concordant with this be entertained, that is, a constitution of a predominantly meteoroidal rather than a gaseous condition, and of a slow rather than a precipitate aggregation, it will, perhaps, appear that the output of heat by the sun in the stages concurrent with this autogenic life period of the earth may have been small. The autogenic thermal era of the earth may thus have corresponded to a period of slight thermal loss by the sun.

As time went on the ingathering of the terrestrial meteorites gradually became more and more distant from one another (since the scattered material was progressively exhausted by previous infalls), while the central or solar aggregation was yet only in its early stages and was gradually increasing in heat. If this increase was in a ratio somewhat proportionate to the decline of the autogenic heat of the earth an equalizing compensation might result, and the earth gradually pass from the relatively independent autogenic thermal stage to the dependent solar stage which has continued to the present. Thus, by the prolonged coincidence of increase on the one side with decrease on the other, the life history of the earth may have been transferred from meteoroidal to solar dependence without such a radical disruption of continuity as to have been generally destructive.

This speculation may seem at first thought to be far-fetched, and to be poised on a ticklish combination of conditions, and it may, indeed, prove, when critically studied, to be really so, but yet it is submitted that it follows along coherent lines connected

ultimately with the fundamental proposition that dispersed meteoroidal matter might gather in slowly rather than precipitately. On this point hangs all the law and the prophets.

If astronomers, physicists and mathematicians will jointly attack the formational history of the solar system stage by stage, following each stage out into details of time and rate, and taking full cognizance of all the alternatives that arise at each stage, it will then be possible, perhaps, to decide whether the conditions of the early earth were such as to require a large or a small amount of heat from the sun for the sustenance of life, and whether the sun was wasting heat prodigally in those days or conserving it for later expenditure. The present measure of the earth's needs may be no measure of its early needs. The sun's present expenditure may be no measure of its early expenditure.

In view of all these considerations, I again beg to inquire whether there is at present a solid basis for any 'sure assumption' with reference to the earth's early thermal conditions, either internal or external, of such a determinate nature as to place any strict limitations upon the duration of life.

The latter part of the address is concerned with novel suggestions regarding the behavior of the supposed liquid surface of the earth in the stages just preceding its final solidification, involving a theory of the formation of the primitive surface rocks and of the original continents and ocean basins. The discussion of this I must leave to the petrologists, merely venturing the hint that they may find some occasion to reconstruct current petrological doctrines if they are to be brought into consonance with the new views offered.

The point of greatest general interest in this part of the address is the sharp statement of opinion that, if the original lava ocean had solidified equably in all its parts and produced a dead-level surface all around the globe, there seems no possibility that our present continents could have arisen to their present heights, or the ocean basins have sunk to their present depths, during twenty or twenty-five million years, or during any time however long. (Exact words previously quoted, p. 897.) Lord Kelvin adds: "Rejecting the extremely improbable hypothesis that the continents were built up of meteoric matter tossed from without, upon the already solidified earth, we have no other possible alternative than that they are due to heterogeneousness in different parts of the liquid which constituted the earth before its solidification" (this JOURNAL, p. 706). This is as strong an assertion of the necessity of assuming crustal and sub-crustal heterogeneity as any advocate of a slow-accretion earth could wish. If the word 'liquid' and what follows be stricken out, and the words 'meteoroidal aggregate' be substituted in the sentence quoted, it will be a rather too strong statement of the alternative explanation which springs obviously from the meteorological hypothesis herein urged. It is not easy to see how such heterogeneity as is required to account for the continents and ocean basins could arise from a white-hot liquid-surfaced earth descended from a gaseous earth. To those who do not follow the petrological conceptions of the address, but who conceive the hypothetical lava ocean to have been one great *solution*, stirred by convectional and other currents and depositing crystals as supersaturation arose from change of temperature or from change in the solution itself, there seems not much more reason to suppose that its deposits would have been localized persistently on the sites of the present continents than to suppose that the present enveloping solution—the ocean—if duly concentrated, would localize in a

similar way the crystals which it would throw down. But this must be left to the petrologists. I cannot, however, express too strongly my appreciation of the value of Lord Kelvin's stalwart opinion respecting the incompetency of the thermal theory of crustal deformation, since this carries with itself, more remotely and occultly (*pace* Kelvin) an implication of like weakness in the theory of the white-hot earth itself.

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*A DANGEROUS EUROPEAN SCALE INSECT NOT HITHERTO REPORTED, BUT ALREADY WELL ESTABLISHED IN THIS COUNTRY.**

In view of the activity and zealousness displayed by several of the European states in excluding American plants and fruits on the pretext of possible contamination with the San José scale, it is opportune perhaps to call attention to the fact that a dangerous and perhaps very dangerous European scale insect, *Aspidiotus ostreæformis* Curtis, has recently become well established in this country. This scale insect is very similar to the San José scale in general appearance and habit, and is liable to be almost, if not equally, as mischievous, judging from the examples of badly infested material which have come to this office for determination.

Aspidiotus ostreæformis is a well-known pest on various fruit trees in Europe, where it has a very wide distribution, but, strangely enough, in view of the ease of its importation on nursery stock, seems not to have gained lodgment in this country until comparatively recently. The first examples of *ostreæformis* coming to this office from American sources were naturally confused with other species, being identified either as *ancylus*, *juglans-regiæ* or *forbesi*, all near allies; and while *ostreæformis* has undoubtedly been established in New York and Ohio for eight or ten years, and in other localities for shorter periods perhaps, its existence in this country has not hitherto been reported in print, and specific identification has only been established within the last year. In fact, so little is the species known that two experts in Coccidæ were prepared recently to describe as new an example, referred to below, coming to Dr. James Fletcher from Pocum, British Columbia. The fact that this material represented the European *ostreæformis*, now first shown to occur in this country, was fortunately determined by Mr. Theo. Pergande in time, I believe, to prevent the publication of the new species.

Within the last few months material representing *ostreæformis* has come to this office and been determined by the writer very frequently, especially from the State of New York, where it seems to have become well established, notably in the vicinity of Geneva. No less that 15 lots of this scale insect have been determined from Geneva, N. Y., representing as food plants plum, cherry and apple. Most of this material has been communicated by Mr. G. G. Atwood, either direct or through Mr. Felt. In one case the food plant is designated as 'European plum,' and the statement is made by Mr. Atwood that the scale occurs in numbers on this food plant, to its considerable injury. Additional localities in New York are: Rochester, on apple (H. C. Peck and V. H. Lowe); same locality, on plum (Dr. Peter Collier), and Millbrook, on pear (E. C. Butterfield, reported as badly infesting 7 pear and 12 plum trees imported eight years before from a German firm); Penfield, on apple (Felt); and also

* The scale insect on pear and apple at Alameda, Cal., collected by Mr. Koebele and determined by Professor Cockerell as *ostreæformis* (Bul. 6, Tech-Ser., Div. Ent., U. S. Dept. Agric., p. 19.), is a case of wrong identification; the species is *juglans-regiæ*, as I have determined from examination of the original material.

from New York State without locality (Felt).

Two localities in Ohio have furnished this scale insect, namely, Wooster, on plum, (F. M. Webster), and Cleveland, on pear (J. A. Stevens). One locality is represented in Michigan, namely, South Haven, on apple, communicated by W. B. Barrows.

In Canada the scale occurs on prune at Pocum, British Columbia (J. Fletcher), and on plum at Niagara, Ont. (Joseph Healey).

A careful examination has been made of all material received at this department representing allied species or those with which there was any possibility of confusion with *ostreæformis*, with the result of the discovery of some half a dozen examples of the latter species. The material earliest received referable to *ostreæformis* bears date of January 12, 1895, and was communicated by Dr. Peter Collier, of the Agricultural Experiment Station at Geneva, N. Y. It was reported as occurring on plum near Rochester, and was identified at that time as *ancylus*, perhaps its nearest American ally. In the same year, April 3, 1895, specimens on plum were received for identification from Mr. F. M. Webster, Wooster, O., and were also referred to *ancylus*. No additional material of this species was received until 1897; in that year Professor W. B. Barrows sent it from South Haven, Mich., and the insect was doubtfully identified as *ancylus*. During 1898 it was received from several localities, as follows: Cleveland, O., on pear; Millbrook, N. Y., on pear; Geneva, N. Y., on plum; British Columbia, on prune, and Niagara, Ont., on plum. In 1899 it was received many times, principally from Geneva. N. Y., and also from Rochester and Penfield, as noted. Its origin on European stock is plainly indicated; possibly the original importation occurring about 1890, although perhaps earlier.

The writer has made mounts and careful studies of this insect from various European localities,and has determined that Signoret's species, *Aspidiotus spurcatus*, is a synonym of *ostreæformis*, and that *A. zonatus* Frauenf., is also probably a synonym, or, perhaps, a mere variety of the same species.

The European localities from which the writer has examined specimens of this scale insect are as follows:

Geisenheim, Germany, on apple, communicated by Dr. L. Reh, Station fur Pflanzenschutz, Hamburg; Isle of Langenau, Nackenheim, Rheinhessen, Germany, on pear sent by Dr. J. Ritzema Bos; Stettin, Prussia, on apple, collected by Mr. Theo. Pergande in July, 1898, 'rather scarce'; Wanganingen, Holland, on pear, sent as probably *perniciosus* by Dr. J. Ritzema Bos; Prague, Bohemia, on *Prunus domestica*, from Mr. K. Sulc; Chester, England, on plum, communicated by Mr. Robert Newstead and labelled as determined by Mr. J. W. Douglas; Florence, Italy, on *Populus tremuloides*, determined as *Aspidiotus spurcatus* (Cherm. It., Fasc. I., No. 3); Italy on *Platanus orientalis*, determined as *Aspidiotus spurcatus* (Cherm. It., Fasc. I., No. 5). On May 19th also, of this year, this scale was found associated with a *Mytilaspis* sp., on cuttings of date palm collected for the Department in Algeria by Mr. Walter T. Swingle.

The species was originally described by Mr. Curtis from pear in England. Mr. J. W. Douglas reports it also in England on plum, pear, apple and cherry, and Mr. A. C. F. Morgan gives the additional food plant, *Caluna vulgaris*, in Portugal, finding it associated with *Mytilaspis pomorum*. On the continent of Europe it has been variously reported as affecting the fruit trees mentioned above.

Aspidiotus zonatus was originally described from specimens found on oak in Vienna. The females occur for the most part on the bark; the males on the leaves. It is widely distributed in Europe. The Department

collection contains specimens from Chester, England (Newstead); and on the white oak from Stettin, Prussia (Pergande).

Aspidiotus spurcatus has been reported from France and Italy on poplar and Platanus.

It will be noted that the Department and other records exhibit not only a wide range of food plants, but a very extended distribution in Europe, both geographically and as to climate. This scale insect, therefore, seems to be one well worthy of attention and one that will bear watching. It is to be hoped that it will not be as disastrous to our fruit interests as have been other foreign scale insects imported to our shores.

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U. S. DEPARTMENT OF AGRICULTURE.

NOTE: After returning the proof of the above to SCIENCE, part of the type material of a supposed new species of *Aspidiotus* (*A. hunteri*), found in 1897 on currant at Alton, Iowa, was sent to this department by the describer of the species, Mr. Wilmon Newell, Assistant Station Entomologist, Iowa Experiment Station. The material in question proves to belong to *ostreæformis*, and is very interesting as showing the occurrence of this species so far west and also as indicating a new food plant.

CROSS-EDUCATION.

THE term 'cross-education' is used to express the theory that the effects of practice on one side of the body are transferred to the unpracticed side. The subject has been investigated during the past year at the Yale Psychological Laboratory in the effort definitely to establish the fact of transference of practice and to arrive at an explanation of the causes of such transference. Following is a brief summary of the experiments carried on and the results obtained from them:

a. Rapidity of voluntary effort.—A tap-counter was constructed from clock-work and connected electrically with a telegraph key. At each pressure of the key by the hand or the foot the counter registered one tap. Records of maximum rapidity of tapping were taken for right and left index fingers and right and left great toes separately. Then for two weeks the right great toe alone was practiced in tapping daily for a considerable time. Then all four digits were tested as at the start. The result for six subjects showed that the average relative gain for the right great toe—the member practiced—was 31% ; for the left great toe, 30% ; for the right index finger, 20% ; for the left index finger, 28%. The last three had, therefore, gained by practice of the first.

b. Strength of voluntary effort.—Six subjects were tested as to the number of times they could raise a dumb-bell weighing $2\frac{1}{4}$ kilos (5 lbs.). Girth measurements of the right and left arms were taken and the dynamometric pressure of each hand was determined. For two weeks the right arm alone was exercised in raising the dumb-bell. Results: (1) The average gain of the right arm in the number of flexions made was 470%; of the left arm, 150%. (2) The average gain in the girth of the right biceps was $6\frac{1}{3}$ mm.; of the left biceps, $2\frac{5}{6}$ mm.; of the right forearm, $4\frac{5}{6}$ mm., and of the left forearm, $2\frac{1}{6}$ mm. (3) The average dynamometric pressure increased in the right hand 13%, in the left 13%. (4) Practice of the *right* arm inured *both* arms to resist the after-effects of violent exercise as revealed by stiffness, pain and soreness. These experiments proved not only the fact of cross-education in ability to do work, but also the fact of cross-development, in a lesser degree, of the symmetrical muscles.

c. Accuracy of voluntary effort.—A target was so devised that permanent records of accuracy in lunging with a fencer's foil could be obtained. Records of both right and left hands were secured with six subjects. The lunging was then practiced for two weeks with the right hand only. Thereafter both hands were tested. Results: (1) Both hands had gained in accuracy,

the right 52%, the left 36%. (2) In both right- and left-handed lunging, the body had gained in grace and coordination of movement. (3) The probable errors of both hands had been markedly decreased as the result of the practice of the right hand.

Cross-education may be the result chiefly of changes wrought in the central nervous system. In the tapping, in which a minimum of muscular strength is required, the gain is about equal on both sides. In the test of strength a smaller proportional gain was found in the side not practiced. The transference of peripheral effects cannot be ignored altogether, since in the dumb-bell test there was a decided increase in the girth of the arm not practiced, and in its power to resist fatigue.

The facts may be explained as the result of two factors: (1) the close nervous connection, through motor centers, between symmetrical muscle groups on opposite sides of the body and between groups related in function or position; (2) the development of general will power and attention, through the practice of one form of volition.

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SCIENTIFIC BOOKS.

The History of Mankind. By FRIEDRICH RATZEL. Translated from the second German edition by A. J. BUTLER, M.A. With introduction by E. B. TYLOR, D.C.L., F.R.S. London, Macmillan & Co. 1898. Colored plates, maps and illustrations. Volume III. 8vo. Pp. 599.

The first edition of Ratzel's 'Völkerkunde,' which has been in our libraries for ten years, is divided into three substantial parts or volumes, the third of which is taken up with the more cultured peoples that are outside of the age of steel and steam. For some reason or other, the order of the work has been changed in the English edition. The higher American peoples are thrust out; Africa obtrudes itself into Volume III., so that it really commences on page 149. It is with the remaining pages that we have here to deal.

The proper appreciation of this volume and, indeed, of the whole work, demands of the reader familiarity with his Peschel, Friedrich Müller, Brinton and Keane, and he would do well to have near by a set of Standford's Compendiums. It is not a treatise on ethnology—the work would be vastly improved by a few tables showing the connection and affiliation of peoples —but a discussion of human artificialities in relation to certain culture areas.

The eastern hemisphere presents to the student a number of arenas on which the drama of mankind has been enacted. On these, races come and go, but Nature's life repeats itself and the forces of progress and reaction are imperceptibly active in them.

In speaking of this play of culture-influences between Africa and Asia the author develops the Erythræan, or Red Sea group of peoples. In contrast with Dr. Brinton's emphasis upon the Hamite and West African forces, Ratzel looks eastward for the predominating influences.

The arena for the sharp conflict between nomad pastorals and settled tillers is found in a broad strip of territory extending diagonally from 10° to 60° north latitude and from the Atlantic to the Pacific.

India is for the author a region where races have been broken up, pulverized and kneaded by conquerors. Doubtless a pre-Dravidian negroid type came first, of low stature and mean physique, though these same are, in India, also the result of poor social and economic conditions. Dravidians succeeded negroids, and there may have been Malay intrusions, but Australian affinities are denied. Then succeeded Aryan and Mongol, forming the present *pot pourri* through conquest and blending.

Northward of India, as suggested, the settled Iranian and the nomad Mongol furnish to the author the best opportunity to study and develop the thought ever dominant in his mind. In the history of mankind the lots fall diversely, but to each race its task is assigned, and none is left without opportunity of casting its threads into the great fabric.

In southeastern Asia, Ratzel sees a great

Transgangetic family of language, with older and more recent members, the former squeezed into the sea board and the mountains, the latter spreading over the interior and along the streams to the deltas.

The Far East gives to the author his best perspective—in the remotest past, a rude stone age no better than that across the Pacific, in California; after that, three thousand years B. C., a bronze age; and then the gradual but victorious progress of the race, its customs and institutions. Japan and Korea are daughter races of Chinese culture. The closing section of this Asiatic portion of the volume is devoted to Asiatic forms of belief and systems of religion. The necessity of religion is assumed, and, as to its forms in the arenas mentioned, "they have their roots in a subsoil of widely diffused notions, in which even now leaves, flowers and seeds, fallen from the lofty trees, are reposing, dying, decaying, germinating."

Ratzel is not in ecstasies over the blessings of the age of iron and machinery. We are liable, he thinks, to overestimate the effect of metals in promoting culture. "The discovery of smelting and forging does not form an epoch. The spiritual foundations of our culture had no workers in steel."

So, the Europeans receive only a passing notice on the last few pages and are handed over to the historian.

In a work upon which the author has expended so much care and erudition one could wish that he had made more concessions to the reader. Few persons are learned enough to read the volume before us. If they desire to consult the authorities named, it is nearly unpractical, and the translation is not so helpful as the original. The illustrations are superb; they embellish and illuminate the work, but they do not greatly illustrate it. For example, the Kha flute, on page 370, after Harmand, finds no explanation for its strange combination of direct flute and reed instrument, and no example is in the national collection. As a summary of culture, however, among the peoples of the eastern hemisphere, still in the epoch of handicraft, Ratzel's third volume is not only vastly superior to such books as Wood's, which is saying little, but it places the author in the front rank among the students of culture-progress, whose pioneers were Klemm, Lubbock, Tylor and Morgan.

O. T. MASON.

SMITHSONIAN INSTITUTION.

Canada Experimental Farms Reports 1891–1898, Vols. 8, pp. 348, 289, 355, 422, 426, 474, 449 and 429. Illustrated.

The system of Experimental Farms of the Dominion of Canada was inaugurated in 1887, with the establishment of the Central Experimental Farm at Ottawa. Since then, as parts of the system, branch farms have been located at Nappan, Nova Scotia, for the Maritime Provinces; Brandon, Manitoba; Indian Head, Northwest Territories, and Agassiz, British Columbia. Each of the branch farms is under the direction of a Superintendent, who reports to the Director at the Central Farm, and he in turn to the Minister of Agriculture, the annual report being issued as an appendix to the report of the Minister of Agriculture. The organization of the Central Farm is somewhat like that of the Experiment Stations in this country, and the staff during most of the period covered by the above reports consisted of William Saunders, Director; James W. Robertson, agriculturist; John Craig, horticulturist; F. T. Schutt, chemist; James Fletcher, entomologist and botanist, and A. G. Gilbert, poultry manager. A foreman of forestry, W. T. Macoun, since made horticulturist, was added to the force during the period covered by the report for 1897.

At the several farms many lines of useful work are carried on, such as scientific investigations, practical field work, the study of forestry problems, etc., different problems being investigated according to the immediate needs of the farming community, but at the Central Farm the greater part of the more important scientific investigations are carried on, this institution being especially equipped for the purpose. In addition to the duties already outlined, the Central Farm has charge of the introduction and distribution of seeds and plants, a few thousands of dollars being annually expended in purchasing and distributing seed grain and forest trees and tree seeds.

The reports of the Experimental Farms give

the results of experiments in agriculture, horticulture and arboriculture, the outcome of prac tical work in the fields, barns, dairy and poultry buildings, orchards and plantations, as well as scientific investigations in the chemical laboratory and the results of studies of the life history of injurious insects and noxious weeds. Variety tests have occupied much of the attention of the agriculturist and horticulturist, the evident desire being to secure the best varieties for the different regions. In this way experiments in the adaptation of certain crops and varieties are conducted upon a scale impossible to the individual, and not a few valuable crops have been secured by this means. Methods of culture and the proper use of fertilizers have been quite thoroughly investigated, to the advantage of the several constituencies. In the treeless regions of Manitoba and Northwest Territories tree-planting experiments have been conducted since the establishment of the branch farms in these Provinces, and, as a result, it is now possible to suggest lists of trees and shrubs adapted for hedge, shelter and timber growth in those regions.

Among results of particular interest and of far-reaching importance noted in the last report is the account of experiments on the effect the plowing under of clovers has on subsequent crops. These experiments have been continued for four years and the beneficial effect of such procedure is plainly shown.

In connection with the variety tests of the agriculturist, attention should be called to the very excellent work done in cross-breeding of cereals. At least two score cross-bred varieties of wheat, and quite a number of varieties of oats, barley and peas have had their origin on the Experimental Farms and some of them seem to be peculiarly adapted to the region, being of more than average productivity and quite resistant to fungus attacks.

The dairy investigations and the experiments in feeding farm animals, especially steers and pigs, have been noteworthy and have led to some very practical results. In nearly every case the feeding experiments were repeated year after year and the conclusions verified.

Among the investigations made by the chemist, the comprehensive survey made of the typical soils of the different Provinces in which their physical characteristics and chemical constituents were determined stands out prominently.

The study of the life history of injurious insects and the investigation of means for combating their attacks have occupied much of the time of the entomologist and botanist. In addition, the subject of noxious weeds, their dissemination and eradication has been investigated. With such subjects as these the efficiency of any method for the destruction of these pests depends largely upon timely warnings which have been given as the emergency arose. Spraying for the prevention of plant diseases has come in for attention and the suggestions given are timely and practical.

The poultry manager has been concerned principally with studying the relative values of different breeds of fowls and their feeding and management.

Throughout all the reports the intensely practical nature of the work is everywhere manifest, the desire apparently being to give results of investigations that may be of immediate use to the farmers and others of the Dominion.

WALTER H. EVANS.
OFFICE OF EXPERIMENT STATIONS,
WASHINGTON, D. C.

BOOKS RECEIVED.

Naturalism and Agnosticism. JAMES WARD. New York and London, The Macmillan Company. 1899. Vol. I., pp. xviii+302; Vol. II., pp. xiii+294. $4.00.

La géologie expérimentale. STANISLAS MEUNIER. Paris, Alcan. 1899. Pp. viii+306 and 56 figures. 6 fr.

Manual of Bacteriology. ROBERT MUIER and JAMES RITCHIE. Edinburgh and London, Young J. Pentland; New York, The Macmillan Company. 1899. Pp. xviii+564.

SCIENTIFIC JOURNALS AND ARTICLES.

THE leading article, in every sense, of the *American Naturalist* for June is that of Sylvester D. Judd on 'The Efficiency of Some Protective Adaptations in securing Insects from Birds.' The author's conclusions, based upon four years' study of the food habits of birds, are that the alleged protective coloration is not the all-important factor in securing an insect from ex-

termination, as some earlier naturalists have supposed. G. C. Whipple and Horatio N. Parker present 'A Note on the Vertical Distribution of Mallomonas.' While the reasons for the peculiar distribution are not wholly apparent, it apparently depends on light and temperature, the organism preferring to live where the light is strong, the temperature low and the water quiet. In an article on 'The Colors of Northern Monocotyledonous Flowers' John H. Lovell considers that the primitive color of the perianth was green, that physiological conditions have often played an important part in determining the coloration of the petals, while insects have contributed to the fixation of such characters when once acquired. William L. Tower records the curious 'Loss of the Ectoderm of *Hydra Viridis* in the Light of a Projection Microscope,' this loss occurring almost completely in from one to eleven minutes. The diagrams illustrating this paper have been transposed. The editor makes the welcome announcement of the forthcoming publication in the *Naturalist* of a series of synoptical tables for the determination of American invertebrates.

Bird Lore for June commences with an all-too-brief note by Frank M. Chapman on 'Gannets on Bonaventure,' accompanied by a full-page plate showing the nesting gannets on one of the ledges. Florence A. Merriam concludes her article on 'Clark's Crows and Oregon Jays on Mount Hood,' and Mary F. Day gives some excellent observations on the Chimney Swift under the caption 'Home-Life in a Chimney.' William L. Baily shows 'Three Cobb's Island Pictures,' with notes thereon. Ella Gilbert Ives writes of 'The Cardinal at the Hub,' and Thos. S. Roberts has an illustrated 'Catbird Study.' Olive Thorne Miller discusses 'The Ethics of Caging Birds,' deciding that this may be done, if properly done. Fred. H. Kennard tells of the birds of 'A May Morning,' and Mildred A. Johnson of those seen on 'A February Walk.' If one might venture a criticism on *Bird Lore* it would be to the effect that the 'young observers' seem to be getting more than their fair share of space.

Terrestrial Magnetism and Atmospheric Electricity for June. As already announced in SCIENCE, this journal is now being issued from the Johns Hopkins University press, Dr. Bauer remaining as editor-in-chief. In view of the addition of atmospheric electricity to the scope of the journal an appropriation has been made to it from the Hodgkins fund of the Smithsonian Institution. The contents of the number before us are as follows: Portrait of Charles A. Schott, Frontispiece; 'The Beginnings of Magnetic Observations,' G. Hellmann; 'Carte Magnétique de la Sicile,' L. Palazzo; 'The Magnetic Work of the United States Coast and Geodetic Survey,' L. A. Bauer; Über einige Probleme des Erdmagnetismus und die Nothwendigkeit einer Internationalen Organisation,' M. Eschenhagen; 'The Secondary Magnetic Field of the Earth,' A. W. Rücker; 'Remarks upon Professor Rücker's Paper and Wilde's Magnetarium,' L. A. Bauer; 'Biographical Sketch and Portrait of Dr. John Locke,' L. A. Bauer; 'Mean Values of Magnetic Elements at Observatories,' C. Chree; Notes, 'Biographical Sketch of Charles A. Schott.' Activity in magnetic work.

SOCIETIES AND ACADEMIES.

GEOLOGICAL SOCIETY OF WASHINGTON.

AT the meeting of this Society, held on May 10, 1899, three papers were read, of which abstracts follow:

Mr. S. F. Emmons read a paper entitled '*Plutonic Plugs and Subtuberant Mountains*,' new terms introduced by Professor I. C. Russell in two articles in Volume IV. of the *Journal of Geology* (1896), to designate hitherto unobserved geological phenomena, the one being a new form of igneous intrusion distinct from laccoliths, the other a new type of mountains. The latter, to which his second article is devoted, are dome-shaped mountain uplifts with granitic cores, which he considers to have resulted from the vertical upthrust exercised by the intrusion of a larger plutonic plug (or *tuber*) beneath their center, and are called by him 'subtuberant mountains.' The idea of vertical upthrust had already been advanced by Dutton in his article on Mt. Taylor, N. M., in which he stated that all the mountain uplifts between the Great Plains and the Sierra

Nevada had been produced by rising granite cores, which might or might not have reached the surface or been exposed by erosion. So far as facts in support of this theory were instanced by Professor Russell for regions that had come under the speaker's observation, Mr. Emmons said they were not correctly stated or interpreted, and that Russell had apparently taken his ideas with regard to the Colorado mountains rather from the early reconnaissance observations of Hayden than from what had been written since in the light of modern geological research. It was only during the past summer, however, that an opportunity had presented to verify the personal observations in the northern Black Hills, upon which Russell based his original discovery. A detailed survey of this region is in progress by members of the U. S. Geological Survey, under Mr. Emmons's general supervision, the results of which, though not ready for publication, show that Russell's own observations, which were confined to three outlying groups of hills, were inaccurate. His supposed plugs are either laccoliths or remnants of laccolithic sheets left by erosion. In describing the other occurrences to support his plug theory Russell had relied mainly on the reconnaissance observations of Winchell and Newton made 25 or 30 years ago, before laccoliths were known, and paid little attention to later observations. In point of fact, the region presents a most remarkable variety of typical laccoliths, and nothing corresponding to the supposed plutonic plug has yet been observed there. Russell's further statement in support of the plug theory, 'that dikes and faults are wanting in the region,' is equally without basis of fact. Crosby, in the article casually referred to by Russell, speaks of the abundance of dikes in certain localities, and observation has shown that both dikes and faults are so abundant in the central mining region that they could hardly escape notice.

The speaker thought such hasty generalizations were objectionable as establishing an undeserved priority in terms that when accurately defined and applied to observed phenomena might be useful to field geologists.

This paper was followed by one upon *Laccoliths and Bysmaliths*, by Walter Harvey Weed, the subject being a sequel to that previously discussed. The facts upon which the paper was based related mainly to the igneous intrusive mass of the Little Belt Mountains, of Montana, in which careful areal surveys have been made, but supported by observations made for ten years past in neighboring parts of the State. These intrusions commonly occur in Cambrian shales, and rest upon crystalline schists. They present gradations from intrusive sheets to laccoliths, and from these to asymmetric laccoliths. Incidentally, it was shown that the asymmetry is due to the general range uplift, furnishing a line of weakness along the limb or monoclinal of the fold. Several of the intrusions are, however, unlike the laccolith, unless we assume an asymmetry about the entire circumference. In other words, faulting and uplift, and not folding, is the prevailing structure. As the field observations show the same spreading out on a definite floor as in the case of the laccolith, these intrusions are not stocks or the so-called plugs of many writers, a term lately revived under the title of Plutonic Plug by Russell. Moreover, gradations occur between them and the asymmetric laccolith.

For such intrusions Professor J. P. Iddings has recently proposed the name of *bysmalith*. It is clearly a form heretofore embraced under the more general term of 'stock.' Its usefulness consists in its affording a definite name for a definite type of intrusion, of which several examples have now been observed.

From a study of the facts observed in the field, it appears that there is a definite relation between these different forms of intrusion, and the form is a function of several factors. The other factors being equal, and lines of weakness absent, a bysmalith has a larger floor area than the laccolith, which accords with the hypothesis that owing to viscosity of the intruding magma the pressure in large masses is imperfectly transmitted laterally, resulting in an increased upthrust producing faulting and the punching upward of the strata. Such intrusions have been described by Davis and Lindgren, though no name was given them. As the term laccolith is preferable to stone cistern, so bysmalith is preferable to plug-stone, both having special meanings not implied by the English transla-

tions of the names, especially as the term plug as used by geologists, including Russell, does not imply lateral expansion.

The third and last paper was by Mr. J. A. Taff. Mr. Taff's observations on '*Changes in the Canadian River in Western Choctaw Nation, Ind. Ter.*,' brought out facts showing that this river once flowed from where it now crosses the Choctaw-Chickasaw line southeastward, well into what is now the hydrographic basin of Red River; that the present river has eroded its bed 100 feet below its old channel; that the old river was 1 to 3 miles wide and had filled its channel with sand, as the present river has done. The migration of the Canadian northward was shown to be, most probably, due to capture by a tributary of Little River by headwater erosion along the strike of friable beds of sandstone and shale. The old channel of Canadian River was surveyed and mapped for 50 miles.

WM. F. MORSELL.

MAY 15, 1899.

TEXAS ACADEMY OF SCIENCE.

THE annual June meeting of the Academy was held at Austin on the 12th inst. The following papers were presented:

1. Some Theorems in Geometry: Dr. W. H. Bruce, Athens, Texas.
2. Southwestern Texas: William Kennedy, Austin.
3. The Ecology and Embryology of the 'Rain Lilies:' Felix E. Smith, Austin.
4. 'An Annotated Record of the Geology of Texas for the Decade Ending December 31, 1896,' with remarks: Dr. Frederick W. Simonds.
5. A Case of Fistula on the Neck of an Adult Man: Dr. W. W. Norman.
6. The Behavior of Certain Caterpillars: Dr. W. W. Norman.
7. Life Zones and Crop Zones in Texas: Dr. William L. Bray.

The election of officers, which occurs annually in June, resulted in the following choice:

President, Dr. Frederic W. Simonds, University of Texas.

Vice-President, R. S. Hyer, Regent of Southwestern University, Georgetown, Tex.

Treasurer, Professor T. U. Taylor, University of Texas.

Secretary, Dr. William L. Bray, University of Texas.

Librarian, Dr. W. W. Norman, University of Texas.

First Member of Council, H. L. Hilgartner, M.D., Austin, Tex.

Second Member of Council, Professor J. C. Nagle, Agricultural and Mechanical College, College Station, Tex.

Third Member of Council, Dr. H. W. Harper, F.C.S. London, University of Texas.

The office of Librarian was created by vote of the Academy, and the Librarian made *ex-officio* member of the Council. The Academy library, consisting thus far of valuable exchanges, is assuming gratifying proportions.

WILLIAM L. BRAY, *Secretary*.

TORREY BOTANICAL CLUB, MAY 31, 1899.

ON the part of the Committee on Nature Study, Miss Sanial described briefly the use of plant material in the vacation schools of New York City, and the need of donations of fresh flowers and other natural objects. They are used for study and for brush work. Many of the children have never seen any wild flowers whatever. Any one who will write to the Board of Education, labeling the communication 'For Vacation Schools,' will receive the necessary blanks for forwarding, and such contributions of plant material are earnestly desired.

Dr. Arthur Hollick followed with a brief abstract preliminary to a paper entitled 'A Comparison between Geological Sequence and Biological Development in the Vegetable Kingdom." He alluded to the first occurrence of modern genera in the Mesozoic, and of modern species in the Tertiary; and to the vigorous growth made by lower forms of algæ in the hot waters of Yellowstone Park, suggesting that similar algal life was probably characteristic of the earlier heated waters of the globe. He stated that many of the Cambrian casts claimed to represent algæ are undoubtedly rightly interpreted; and then sketched the successive appearances of the earliest known gymnosperms, in the Devonian, monocotyledons, in the Triassic, and dicotlyedons in the Cretaceous, by the middle of which period many modern genera are recognized. Ferns and Lycopods of modern families appeared in

the Devonian, the first known Musci, Hepatica and Fungi in the Tertiary. Plant remains in glacial deposits are exactly the same as species now living a little farther to the north. The Carboniferous fern-species which have been figured and named outnumber those of the whole world now living. The coal flora was probably practically identical all over the world. Every time a new horizon is opened up, even down to the Tertiary, there are many new fossil ferns discovered in it. A species in paleobotany simply means a description of a certain organism. We may find that some or many of these actually belong to the same species.

Discussion followed, in which Dr. Underwood, Mr. Eugene Smith and the Secretary participated. Dr. Underwood called attention to the descent of the ferns, not from the mosses, but probably from earlier generalized ancestors of both ; and spoke of the disparity in numbers between the fossil and the living ferns of Pennsylvania—45 living, but at least 375 fossil—and and asked : "How many of the 45 now living in Pennsylvania are at present being preserved in sediments?" Many of them are seldom found above ground, to say nothing of their occurrence beneath.

The second subject presented was the exhibition and description of a hygroscopic plant-specimen by Dr. C. J. Eames. The specimen was originally described in an article entitled 'The Resurrection Flower' in *Harper's Monthly*, April, 1857, p. 619. Dr. Eames' specimen seemed as if it were the ripened circle of ovaries of some malvaceous flower, and displayed very marked hygroscopic movement, expanding completely within fifteen minutes after moistening. Dr. Eames, a chemist, obtained his specimen in 1860 from Dr. I. Deck, a chemist, who said that he had secured this, and one other like it, about 1849 when in Upper Egypt. The other specimen passed into the possession of Humboldt. Dr. Eames exhibited specimens of *Selaginella* and *Anastatica* for comparison, their hygroscopic movement being less perfect. In the discussion following Dr. Schoeney stated that he has retained *Equisetum* spores which have held their hygroscopic power for ten years unimpaired.

Edward S. Burgess,
Secretary.

BOTANICAL NOTES.

THE POPULARIZATION OF BOTANY.

From time to time attempts are made to popularize some department of science, with less or more success according to the abilities of the author. In this country we have had many illustrations of how not to do such a work, with a few examples which have been successful. Botany has perhaps more than any other science suffered from the attempts of unprepared authors, and, as a consequence, we have had a swarm of books and booklets filled with all kinds of misinformation in regard to plants. It is little, if any, better abroad, but there one finds, now and then, a really good book which is popular in style and yet accurate in regard to its matter. Perhaps the explanation of the latter fact may be found in the other fact that occasionally an eminent botanist undertakes the task of writing for the people. One of the latest illustrations of this is the third edition of Van Tieghem's 'Eléments de Botanique.' That the author is thoroughly prepared to present the subject needs no discussion here, and an examination of the text shows that he has been able to present it in such form as to make it readable to any one of ordinary ability. This result has been attained by the use of vernacular terms, or, where these did not exist, by the modification of technical terms into forms which so nearly resemble the vernacular as to be readily accepted by the ordinary reader. In this manner the author is able to discuss, in successive chapters, topics like the following: the body of the plant, the root, the stem, the leaf, the flower (in all of which the morphology is first taken up and then followed by the physiology), development of the phanerogams, formation of the egg and development of vascular cryptogams, formation of the egg and development of mossworts, formation of the egg and development of thallophytes, development of the race. In the second part of his book the author boldly takes his readers through the difficult field of systematic botany, from thallophytes to phanerogams, closing with a chapter on the distribution of plants.

We do not have to agree with what we must regard as little better than scientific vagaries in some portions of the author's discussions of the

relationships of certain flowering plants when we express admiration for the general plan and spirit of the work. We do not have to approve of many of the attempts of the author to avoid the use of Latin names of plants in order to be able to say that the book is one to be commended. The author has shown us how a scientific man may write so that the people may read and will read what is written. For this we owe him our thanks. We do not like 'Albuge blanc' for *Albugo candida*, 'Beggiate blanche' for *Beggiatoa alba*, 'Charagne fragile' for *Chara fragilis*, 'Botryche lunaire' for *Botrychium lunaria*, 'Welwitschie admirable' for *Welwitschia mirabilis*, 'Oponce vulgaire' for *Opuntia vulgaris*, etc. On the other hand, many of the author's modifications of the Latin names are quite happy : for example, 'Puccinie du gramen' for *Puccinia graminis*, 'Tilletie du Blé' for *Tilletia tritici*, 'Pézize' for *Peziza*, 'Oedogone' for *Oedogonium*, Zygnème' for *Zygnema*, 'Pteride' for *Pteris*, 'Agroste' for *Agrostis*,' Myriophylle' for *Myriophyllum*, etc. When an American botanist who is prepared to undertake the work sets about the task of writing a botany for the people he will do well first to pretty carefully read Van Tieghem's book.

REPORT OF THE MISSOURI BOTANICAL GARDEN.

THE Tenth Annual Report of the Missouri Botanical Garden has just come to hand, and we are able again to form some estimate of the value of the gift which Henry Shaw made to Science when he set aside a fortune for the endowment of the Garden. From the report of the financial officers we learn that the net available income derived from the endowment after paying taxes, insurance, repairs, etc., is also a little more than $37,000. Of this sum about $21,000 have been used in the maintenance and improvement of the Garden as a collection of plants. About $13,000 have been used in providing for the expenses connected with the scientific work of the Garden, including the herbarium, library, research work and publications. The remaining $3,000 have been used for the Shaw School of Botany, and for the expenses of the annual 'Flower Sermon,' 'Flower Show,' and banquets, which were designated by Mr. Shaw. A careful study of the financial report shows that the trustees are so managing the estate as to increase its value, apparently with an eye to its greater usefulness in the future.

The net results botanically each year are the maintenance of a botanical garden of high scientific importance, and provision for the library, herbarium and publications which pertain thereto. We have now had ten reports, each including scientific papers of a high order of merit, dealing with many phases of botanical work. A few titles will suffice to show the range of these papers, as follows: 'Revision of the North American Species of *Sagittaria* and *Lophotocarpus*,' 'Juglandaceæ of the United States,' 'A Revision of the American Lemnaceæ occurring North of Mexico,' 'A Revision of the Genus *Capsicum* with especial reference to garden varieties,' 'List of Cryptogams collected in the Bahamas, Jamaica and Grand Cayman,' 'A New Disease of Cultivated Palms,' 'Notes on the Grasses in the Bernhardi Herbarium collected by Thaddeus Haenke and described by J. S. Presl,' 'A Sclerotioid Disease of Beech Roots.' A list has been published of the books and papers which have emanated directly and indirectly from the Garden, which shows that during the brief period of its existence no less than two hundred and twenty-three contributions have appeared. Could the generous founder return to see the results of his philanthropy he would doubtless feel that his hopes had been more than realized.

ATLAS OF OFFICINAL PLANTS.

THE second edition of Berg and Schmidt's 'Atlas der Officinellen Pflanzen,' which has been under way for several years, has reached the twenty-fourth *Lieferung* and Plate 140. The work maintains its high degree of excellence, and the plates are particularly to be commended for their scientific accuracy as well as beauty of drawing and coloration. When completed, this work will be of the greatest value to the student of medicinal plants, while at the same time it will be useful to the general botanist.

CHARLES E. BESSEY.

THE UNIVERSITY OF NEBRASKA.

MEASUREMENTS OF ASYLUM CHILDREN.

DR. ALES HRDLICKA has recently published a paper containing a series of very interesting 'Anthropological Investigations on One Thousand White and Colored Children of Both Sexes,' inmates of the New York Juvenile Asylum. The principal aim of these investigations was to learn as much as possible about the physical state of the children who are being admitted and kept in juvenile asylums, while it was also intended to add to our knowledge of the normal child and of several classes of abnormal children. It is well known that a large proportion of the children admitted to juvenile asylums are sent there on account of the poverty of their parents, while another large contingent are committed as incorrigible or even criminal. As both these classes are, from a sociological point of view, abnormal, it is important to learn how far their physical characteristics conform to their moral character, in order to justly decide whether or not they are materially handicapped in their struggle for life, since their treatment and prospects would depend largely on the answers to this question. Dr. Hrdlicka's observations and measurements have a direct bearing on this point, while they are also of value to the anthropologist and zoologist.

While the asylum children are of somewhat smaller stature and smaller weight than were the outside children available for comparison, these deficiencies are probably due to lack of nutrition caused by poverty; measurements of the heads show no great departure from what is considered normal. Criminal and vicious children are not, *as a class*, characterized by any considerable physical inferiority, while the mental ability of at least 85 *per cent.* was equal to the average ability of children outside the institution. Dr. Hrdlicka, therefore, concluded that this class of children make a favorable showing and, with proper treatment, give great hopes as to their future. It is considered of great importance that such children should remain sufficiently long in the asylum to enable them to acquire and retain good habits.

It is found, while the variety of abnormalities existing among the inmates of the asylum is very great, that there is no one abnormality nor set of abnormalities characteristic of the children as a class, and that the characters are usually so slight as not to interfere with any progress the children might otherwise be capable of.

The fact that certain pretty constant differences exist between the colored and white children is of considerable interest, the more that, zoologically speaking, these differences are such as to indicate that the negro is more generalized than the white. Thus the negro children exhibit more uniformity in their physical characters and less tendency to congenital variation, although more succeptible to acquired abnormalities, chiefly the results of rachitic conditions. The ears of many show an almost specific character in having the helix bent on itself and compressed at the highest fourth of the ear; the arms are slightly longer, and in general the bodies of the negro children show less adipose tissue and more muscular development.

All in all, the report deserves to be read with care. F. A. L.

SCIENTIFIC NOTES AND NEWS.

YALE University has conferred the degree of LL.D. on Professor Charles Sedgwick Minot, of Harvard Medical School, and on Dr. Emory McClintock, of New York, lately President of the American Mathematical Society.

HARVARD University has conferred the degree of LL.D. on Professor Arthur T. Hadley, President-elect of Yale University.

HOBART College has conferred the degree of LL.D. on Professor W. K. Brooks, of the Johns Hopkins University.

PROFESSOR NEWCOMB attended the meeting of the Paris Academy of Sciences, of which he is the only American honorary member, on June 12th.

FRANK SCHLESINGER, PH.D. (Columbia), has been appointed an observer in the United States Coast and Geodetic Survey, and will be stationed at Ukiah, Cal., where he will take part in the international plan for the determination of the variation of latitude.

M. HENRI MOISSAN was elected an honorary member of the German Electro-chemical Society at its recent meeting at Göttingen.

Dr. P. F. Raymond, the successor of Charcot in the chair of nervous diseases at the Saltpêtrière, has been elected a member of the Paris Academy of Medicine.

The University of Michigan has conferred the degree of S.M. on Dr. Charles F. Brush, of Cleveland, and Professor W. W. Campbell, of Lick Observatory. They are both graduates of the University of Michigan.

Professor H. A. Pilsbry, of the Philadelphia Academy of Natural Sciences, has received the degree of Doctor of Science from the University of Iowa.

We learn with regret of the death of W. W. Norman, professor of biology at the University of Texas, from typhoid fever at Boston.

At the approaching meeting of the French Association for the Advancement of Science at Boulogne a monument of Duchenne, known for his contributions to electro-therapeutics will be unveiled. The Association will for the first time have a Sub-section of Electro-physiology.

The Maryland Geological Survey has established a laboratory for the physical analysis of soils, and Mr. C. W. Dorsey, of the U. S. Department of Agriculture, has been detailed to superintend the work. A full outfit of apparatus similar to that used by Professor Whitney in the physical determination of soils has been installed, and work will be continued during the coming year upon the soils of Maryland, in conjunction with the geological surveying of the same area. The Survey has further recently had constructed by Mr. Henry J. Williams, of Boston, a very elaborate calorimeter for the determination of the calorific power of coal, preparatory to the investigations of the coal formations of Maryland, which will afford the subject for an exhaustive report at an early date.

We learn from the *American Geologist* that the Minnesota Academy of Natural Sciences will send to the Greater American Exhibition at Omaha a collection illustrating the natural history of the Philippine Islands. The collection will comprise nearly 1,000 birds, a large number of vertebrates, including huge bats and snakes, a collection of shells and corals, and an elaborate ethnographical display.

The observations at the Magnetic Observatory at Vienna have had to be discontinued on account of the electric tramways and electric light wires. *Terrestrial Magnetism* states that the Director of the Observatory, Professor Pernter, has submitted a plan to the Austrian government for a new Observatory, to be situated some distance from Vienna, and to be provided with instruments of the latest construction.

We learn from the *National Geographic Magazine* that Col. W. S. Brackett, of Peoria, Ill., has organized and equipped an expedition to determine the geological and mineralogical features of the almost unknown region lying between Buffalo Hump, in Idaho county, Idaho, and the Nez Perce Pass, in the Bitter Root range. The party numbers twelve men, all experienced mountaineers, some of whom have been in that country since 1862.

The U. S. Fish Commission steamer *Fish Hawk* has arrived at Woods Holl and will be used for biological work throughout the summer.

In compliance with a request of Governor Roosevelt, the Hon. Andrew H. Green, President of the Society for the Preservation of Scenic Historic Places and Objects, has appointed a committee to confer with the New Jersey commission in regard to the preservation of the Palisades. The committee consists of Edward D. Adams, Abram G. Mills, George F. Kunz, Fred. S. Lamb and Edward Payson Cone, all of New York City.

Surgeon J. C. Boyd, of the Navy, one of the United States delegates to the recent Tuberculosis Congress in Berlin, has returned to Washington, and is preparing a report for the Department on the work of the Congress. Dr. Boyd thinks that the results of the Congress are important.

At a meeting of the American Fisheries Society, held at Niagara Falls June 28th and 29th, it was voted to hold the next annual meeting at the Station of the U. S. Fish Commission, Woods Holl, Mass.

Under the auspices of the Royal Horticultural Society of London an International Conference will be held this month on the hybridization of plants. The U. S. Department of Agriculture will be represented by Mr. H. J. Webber.

INVITATIONS have been sent for the Fourth International Congress of Psychology, which will be held at Paris from the 20th to the 25th of August, 1900. The organization is left to the French members, the following being the officers: President, Th. Ribot, professor of experimental and comparative psychology in the Collège de France; Vice-President, Charles Richet, professor of physiology in the Paris Faculty of Medicine; General Secretary, Pierre Janet, Director of the Laboratory of Psychology in the Collège de France. The seven Sections and the Presidents are as follows: (1) Psychology in its relations to physiology and anatomy, Professor Matthias Duval; (2) Introspective psychology and its relations to philosophy, Professor G. Séailles; (3) Experimental psychology and psycho-physics, M. A. Binet; (4) Pathological psychology and psychiatrie, Dr. Magnan; (5) Psychology of hypnotism and related questions, Dr. Bernheim; (6) Social and criminal psychology, M. Tarde; (7) Comparative psychology and anthropology, Professor Ives Delage. Those wishing to attend the Congress should apply to the Secretary, and those wishing to present papers should forward abstracts not later than January 1st, next.

THE Eighteenth Congress of the British Sanitary Institute will be held at Southampton from August 29th to September 2d, under the presidency of Sir William H. Preece. There will be three sections meeting for two days each, dealing with: (1) Sanitary Science and Preventive Medicine, presided over by Sir Joseph Ewart, M.D., F.R.C.P.; (2) Engineering and Architecture, presided over by Mr. James Lemon, M.Inst.C.E., F.R.I.B.A; (3) Physics, Chemistry and Biology, presided over by Professor Percy F. Frankland, F.R.S. There will also be special conferences of municipal representatives, port sanitary authorities, medical officers of health, medical officers of schools, engineers and surveyors to county and other sanitary authorities, veterinary inspectors and sanitary inspectors, and a conference on domestic hygiene.

Nature, quoting from the Allahabad *Pioneer Mail*, states that some important changes are being made in the meteorological department of the government of India. These comprise the abolition of a number of observing stations which have not proved worth keeping up, and the substitution for them of others in more favorable localities. Of the latter, most important are stations which are to be established at Cherapunji and one or two other places in Assam, which will enable a more careful watch to be kept over the meteorology of the tea districts, also regarding the periodical rise and fall of the rivers which are so important for the jute trade. Arrangements are also being made, but are not yet concluded, for the establishment of an observatory on Dodabatta Peak, the highest point in the Nilgiris, which is likely to be valuable in connection with the warnings of the monsoon.

ACCORDING to *The Medical Record*, a young man of Newport, Vt., a student of the University of Vermont, has brought suit against the professor of mathematics in the University for damages for the sum of $10,000. He says that he sustained an injury of the leg as the result of the taking of ten X-ray photographs of his leg soon after the bone had been fractured and while it was healing.

THE French naval authorities, acting in conjunction with Signor Marconi, on June 17th conducted some successful experiments with wireless telegraphy between a ship and the shore in the English Channel. We learn from the London *Times* that the French storeship *Vienne* was used for the purpose. One of Signor Marconi's installations was fitted up on board, and the inventor was present. Wimereaux, near Boulogne, and the South Foreland lighthouse, on the Kentish coast, were used as the land stations. Up to June 17th the distance between the South Foreland and Boulogne, about 28 miles, was the greatest space through which the messages have been transmitted. On June 17th messages were sent between the vessel and the English coast from off Boulogne, and afterwards at intervals, until the vessel was 12 or 14 miles away from that port. The greatest distance through which the messages were telegraphed were 42 miles. The increased distance appeared to have no effect, the messages being recorded at the receiving

station at the South Foreland with unvarying distinctness. The experiments began at 8 a. m., and were continued throughout the day. In the afternoon the Channel was enveloped in a dense fog, but this did not in any way interfere with the transmission of the messages. The vessel was fitted up with a wire passing up the masthead, and messages were exchanged while the vessel was travelling at various conditions of speed with the same result. An interesting feature in the experiments was the facility with which Signor Marconi's latest development for cutting out a station was applied. The messages were sent at will either to Wimereaux or to South Foreland, without the other station being able to intercept them. The results of the experiments are to be reported to the French government.

CONSUL SKINNER, of Marseilles, under date of May 4, 1899, writes to the Department of State that reports from Algeria indicate that standing crops will be seriously damaged and in some cases destroyed by the clouds of grasshoppers now moving in a northerly direction. Ten thousand francs have already been placed at the disposal of the general of the division for the first expenses incurred in fighting against the invasion, and steps have been taken to secure $38,600 additional for the same purpose. Near Biskra 3,200 camels are being employed in the transportation of inflammable material which is being burned where deposits of eggs are found. In all parts of the colony men are at work plowing up eggs and destroying them. It is hoped that the energetic measuses being taken will prevent a now menaced catastrophe. The Algerian wheat crop of 1898 was estimated at 24,118,000 bushels. The exports of cereals from the colony during 1897 were as follows, in tons: Wheat, 54,178; corn, 971; barley, 33,492; oats, 32,781; flour, 2,826.

UNIVERSITY AND EDUCATIONAL NEWS.

WE regret to learn that a decision, handed down by Judge Lacombe, reopens the Fayerweather will by which some five million dollars was bequeathed to educational institutions.

THE Board of Visitors appointed to inspect the U. S. Naval Academy has handed in a report recommending the expenditure of $461,000 on buildings and land.

£10,000, half given by an anonymous benefactor and half appropriated from university funds, will be used for the erection of a pathological laboratory at Oxford.

YALE University, at its recent commencement, conferred 599 degrees as follows: B.A., 294; Ph.B., 136; C.D.S., 22; B.F.A., 2; LL.B., 65; M.D., 7; M.A., 34; D.C.L., 3; C.E., 1; M.E., 3; M.S., 2; Ph.D., 30.

THE Rev. George Harris, D.D., professor of theology at the Andover Theological Seminary, has been elected President of Amherst College.

CHANCELLOR MACLEAN, of the University of Nebraska, has been offered the presidency of the University of Iowa.

DR. E. BENJAMIN ANDREWS has been reelected Superintendent of the Chicago public schools.

DR. JAMES EWING has been appointed professor of pathology in the Cornell University Medical College, and in the University assistant professors have been appointed as follows: Dr. John Gifford in forestry, Dr. B. F. Kingsbury in histology and embryology, and M. V. Slingerland in entomology.

THE following appointments and promotions have also been made: Charles W. Wardner, Ph.D. (Johns Hopkins), to be professor of physics in Williams College; H. G. Byers, Ph.D. (Johns Hopkins), to be professor of chemistry in the State University of Washington; Alfred H. Seal, Ph.D. (Pennsylvania), to be professor of chemistry in Girard College, Philadelphia; J. F. Collins, now curator of the herbarium in Brown University, to be instructor in botany; Howard Opdike, now instructor, to be assistant professor of mathematics at Union College; S. Alfred Mitchell, Ph.D. (Johns Hopkins), to be tutor in astronomy in Columbia University; Dr. Oliver L. Fassig, to be instructor in climatalogy in Johns Hopkins University, and Miss Robinson, of the University of Michigan, to be instructor in biology in Vassar College.